29/09/2019 – Response to PCI recommender and referees for the Manuscript "Deer slow down litter decomposition by reducing litter quality in a temperate forest"

Response to recommender

Dear recommender,

Thanks for the comments you and two anonymous reviewers provided on our paper entitled "Deer slow down litter decomposition by reducing litter quality in a temperate forest".

We carefully revised the manuscript according to the comments and suggestions. By doing so we also put a great effort in editing the text towards a manuscript improved in its structure, flow of ideas and written with much crisper sentences in order to facilitate comprehension. The modifications are detailed below.

Yours sincerely,

Simon Chollet in the name of the authors

Comments from recommender:

After reading carefully your manuscript and the reviewers' comments I concur with them to think that the manuscript is interesting and timely but that some improvements and explanations are required before its formal recommendation.

In particular, I have had difficulties to fully understand the design of the second experiment. I think this is because the ''formula'' line 186-187 is confusing to me (I do not manage to find the same number of litter bags by plot). Similarly it is not fully clear to me whether the feces and leaf litter are in the same bags or not. Moreover, the hypotheses behind this experiment are not clear to me and are not fully explained. As mentioned by the reviewer, the feces are likely to have an overall low impact on the overall litter dynamics due to the relative abundance of feces and tree litter?

- ➔ *"Design of second experiment*": **its revised description is in lines 215-223**
- ➔ *"Formula":* **the formula has been clarified in lines 224 to 227**
- ➔ *"not fully clear to me whether the feces and leaf litter are in the same bags or not":* **litterbag content for experiment 2 has been clarified in lines 215 to 223 and in the formula in lines 224 to 227**

➔ *"feces likely to have an overall low impact":* **yes this is a valid prediction and has been explicitly stated in line 142-143 (prediction 3) and discussed as such in the discussion in lines 551 to 558.**

I have also some questions about the statistics.

Somehow, I am missing ANOVA tables, with comprehensive stat results (for experiment 1, effect of litter source, decomposition place and all interactions).

➔ **OK we now provide the table of ANOVA. Note that we observe the same significant effect of litter quality ("litter origin effect"), little effect of decomposer ability ("decomposition place") and no effect of Home Field Advantage ("interaction between the previous effect"). We added this tables as Supporting Information (Table S3).**

The effect of the mesh size is nearly never mentioned. In some cases, it is even not clear whether it has been tested.

➔ **This has been corrected in the revised manuscript in all sections. The one exception is for C and N mass loss that we decided to only estimate for the fine-litterbags to remain within our budget constrain (as mentioned L245). As we did not measure N and C in the litterbags at the end of the experiments, we can't directly test the effect of mesh size in the ANOVAs.**

The figures mention post-hoc tests but I am not sure the "name of this test" is ever mentioned.

➔ **We have corrected this in the Methods (L258-259) section saying that we used the multiple comparison post-hoc test with the function 'kruskalmc' from the package pgirmess in R**

I am confident that the method to test for the home field advantage is suitable, but slightly more explanations should be given on this method (why not testing the interaction between provenance and decomposition location and testing the hypothesis using the suitable contrast?).

➔ **We chose to use the method developed by Keiser et al. (2014) because it was specifically designed to "(i) create a single, empirical model that explicitly accounts for and estimates the relative influence of litter and soil microbial community abilities on decomposition, as well as the HFA of each home combination; (ii) provide a statistical test of the differences in relative abilities and HFA; and (iii) to allow computing the expected decomposition rate of a single litter x soil combination using the parameter estimates" (Keiser et al. 2014 p607). As suggested we provide additional information on the method in L129-135, L272- 291.**

Fig. 1 describes the plant communities, but could the differences be tested using a "between analysis"?

➔ **We add this analysis as suggested (see L251-252 for methods and L302 for results).**

As the reviewers, I find the use of " C and N loss" confusing. Indeed, these are losses from the litter bags but not necessarily from the ecosystem. Ideally, to describe the impact of herbivores on soil and ecosystem functioning it would be important to assess the proportion of the C and N removed from the

litter bags that is still in the soil and the proportion that is in the atmosphere (OK I know this is very difficult to achieve).

➔ **As suggest by reviewers we corrected the text and specify that it is N and C loss from the litter in the litterbags. We agree that ideally it would be interesting to know to which compartment C and N have moved but as you say this would be very difficult to achieve…**

Comments from referee 1:

General

The topic of this article is well chosen and up to date as it deals with carbon and nitrogen cycle. Indeed the effect of human management on ecosystems, such as re-introduction of wild ungulates, is important to be well known in order to adapt our actions and modify our impact.

All parts of this article are well written, in a good English. The study is well designed and the protocol complete. In the discussion, ideas are well supported by the results and clearly explained. The conclusion brings the most important results as a key home message. I really suggest publication of this study after some clarifications.

Few sentences may not be totally clear to me and I detailed below the points that could be improved.

Abstract

Line 32-33: At the first read, it is not clear what you mean by "reduce carbon and nitrogen loss". Is it from the soil compartment, or the vegetation compartment? When we have not read the article yet, it can be unclear. Just indicate that it is in litterbags.

➔ **We modified the sentence as proposed (see L33-34)**

Introduction

Line 57-58: it seems to me that the situation in Europe and in North America may be different. When you say "farm abandonment and reversion to forest": is it really the case in Europe as well? In France, the development of agriculture offered food to wild ungulates and the hunting practices influence strongly the population of wild ungulates (cf. review of Barrio Garcia et al. 2012). You may complete this idea, or indicate that it is only the case in North America. To be confirmed…

➔ **Actually on the two continents the two trends operate at the same time (agriculture providing more and more subsidies to ungulates in some areas and agricultural abandonment and afforestation in other parts. On both continents major changes in hunting regulations also favoured deer. See rewrite in L51 to 59)**

Line 89-95: The impact of wild ungulates on soils may also affect litter decomposition processes… Some studies related to "soils" and wild boars are of interest and could be cited here. For example: Bueno, Cuevas, Wirthner, Parkes… it is to open to other wild ungulates than only deer.

➔ **We agree that the case of wild boar is a whole other story but beyond our argument. So we clarified our text by narrowing it to deer from the outset and avoiding being side tract by alluding to ungulates. So this has been done throughout the text including the title.**

119-127: This part is not very clear at first read… either some information are missing, or too many information are given. I am waiting the material and methods to clarify.

What means "community scale": please, detail a bit more.

➔ **This terminology was indeed misleading. It referred to the spatial scale experiment 1 was conducted which could be reformulated as "habitat scale". The revised version avoids these kind of misleading expressions.**

It is not clear yet how you choose litter representative of herbivory levels. (I am sure it comes later in the text, but we need some understanding at this point, or not to talk about it yet and wait for the material and method section).

➔ **This is described in L198-202 and L207-211, a rewrite that avoids the gap between the mention of the explanation.**

What do you mean by "home field advantage" in this case?

➔ **We define it in L 89-92.**

Material and Methods

Line 135: what means BEC subzone and CWHwh1… ?

➔ **It refers to the Biogeoclimatic Ecosystem Classification (BEC) of British Columbia, Canada. According to this classification, our study site falls in the subzone Coastal Western Hemlock Wet Hypermartime (code CWHwh1). Explanation and reference was added to the text for clarification (see L152-154).**

Line 149: do you have any information about soil type on each island as it may influence strongly soil decomposition?

➔ **Soil type for the three islands is organic soil that is classified in the folisol order according to the Canadian System of Soil Classification. We added this information in the manuscript (see L156-158).**

Line 211: why 5 mm? We usually use 2mm before soil analyses.

➔ **We choose 5mm over 2mm because it was more adapted for the soil texture we had (soil with high content of organic matter, Haynes & Swift 1990). We explained this in the text (see L187-188).**

Results

Fig.1D: you indicate in the text that soil pH decreased with increasing deer browsing pressure (with a P value<0.05), however, on the figure the letter does not show any significant differences (only "a"). Can you check?

➔ **This is due to the fact that we found a significant difference among pH means with the ANOVA but we don't have enough statistical power to discriminate which mean is different with the post-hoc test. We clarified this in the text L310-311.**

Line 278: I am not familiar with this test, and it needs some more details for better understanding it. Could you precise the unit of the Y axis in the graph 2B if there is one?

➔ **We developed the explanation about the test (see L271-291). The Y axis is without unit as it represent an effect size (the idea is to identify difference from 0 of the parameter).**

Line 283: can you remind here what means "litter quality". I had to go back to material and methods.

➔ **We added to the manuscript that litter quality "Litter quality, understood here as the rate of decomposition independent of decomposer ability and home field advantage [calculated using the method developed by Keiser et al., (2014)] Please see L339-340.**

Line 291: you do not say anything about fig 2D litter quality… just one sentence to indicate that it was significant…

➔ **We added this to the manuscript: "Similarly than for carbon, litter quality (sensu Keiser et al. 2014) was the main driver of nitrogen loss in litter bags. However, conversely to carbon, the lowest rate of N loss after one year was observed in litter from the island with intermediate browsing pressure (Figs 2E and 2H)". Please see L381-384.**

Line 308: table S3: what means Rao litter?

➔ **It was a mistake, we removed it.**

Line 319: in table S3, is there any data concerning the coarse mesh litterbags? Where can we see the 26 and 38% lower values? Ok, I just find out this result in fig S2. Please just indicate that it comes from figure S2 on line 322.

➔ **Done**

For Fig S2 I would add mass loss in fine mesh litterbag (and coarse mesh litterbag) on the Y axis for more clarity.

➔ **Done**

Line 338: are you sure that it is seen on Fig S2? I do not see any results about feces on fig S2. It should be S3?

➔ **It was indeed Fig S3, this mistake has been corrected.**

Discussion

Line 392-395: I am not very convinced by this explanation. On fig. 1B, we do not see that CWM litter C:N ratio is higher for intermediate pressure. Can you check this point?

➔ **Although CWM litter C:N ratio was not significantly different across herbivory treatment, the highest CWM litter C:N values were found in the intermediate browsing pressure treatment (as visible on Fig.3). Figure 1B only shows the mean value and standard deviation, which are similar across treatments. Figure 3 however shows the distribution of the CWM C:N across the plots, and we can see on this figure that highest value (above 80) are only found in the intermediate treatment. As suggested in your next comment, we added the reference to Figure 3 and clarified this point in the text.**

Line 404: it could be useful to rewrite the figure where we can see each result you indicate in the discussion. It allows the reader to go back to the good graph directly.

➔ **We updated the manuscript accordingly with this suggestion and made reference to figures more explicitly throughout the text**

Line 412: I am sure this hypothesis is based on literature. Can you cite on article related to this point? Does Prescott, 2010 talk about this? Just indicate it in the text earlier.

➔ **Yes Prescott 2010 mentioned that link between nutrient availability and the formation of mor humus. We mentioned the reference earlier.**

Comments from referee 2:

Review PCI Ecology

Large herbivores strongly slow down litter decomposition in temperate forests by Chollet et al.

Using a 1yr long litterbag transplant experiment in three islands of the Haida Gwaii archipelago capturing a range of deer density/history, this paper addresses the question of topdown controls of large herbivores on ecosystems, and more specifically whether and how much deer density/presence affects litter decomposition directly and/or indirectly in temperate forests. The main result of this work is that deer population density affects C and N loss from litter decomposition primarily through plant community changes, and secondary by changing abiotic soil conditions and decomposers community ability, or

through feces deposition. I particularly appreciated the demonstration of the key role of the soil macrofauna in changing the HFA results.

Yet, as they are presented in the ms, the results do not fully or clearly support all the conclusions. As you will see in my detailed comments underneath, I first worry that the sampling design might be limiting the reach of the conclusions with only one sampling event after one year of decomposition.

➔ **We agree that ideally it would have been interesting to study the effect of deer on the kinetic of decomposition, but the budget and logistic constraints were against us. Access to the sites is a logistical difficulty at all times but even more so as winter weather is unpredictable and very challenging restricting access. Furthermore repeat visits would have meant multiplying the number of litterbags to match the number of visits, multiplied the time to spend in the field (one month for the current set up) and to multiply time and costs related to sample treatment and analysis. While the uniqueness of the context we used is a real asset it comes with the above mentioned limitations that would be extremely challenging to come by. However we believe that our study design allows to answer the questions we posed, namely: do deer modify mass, C and N loss from litter after one year of decomposition? We indicate the reference to the time of decomposition throughout the manuscript to make this clearer. See L 230-232.**

Second, the standard litter used in Exp.#2 was probably not adequately chosen.

➔ **What is the reason that would make it inadequate? *Picea sitchensis* is one of the dominant tree on the studied islands and is present at all study sites. Even if the proportion of spruce in litter is higher on islands with deer they are not foreign for decomposer communities from islands without deer. In addition this species is widespread across treatments making it, we believe, a valid choice to compare effect of feces addition.**

Third, the patchiness of the dung/urine might explain why they had no effect on global decomposer ability, but this questions the sampling intensity (# of litter bags deployed) and its capacity to capture the microclimatic, topographic and biochemical heterogeneity of these browsed islands (strong and intermediate).

➔ **We agree with the first part of the comment. We think that the coupling of our two experiments actually allows to produce at the same time the overall pattern of decomposition at a broad scale and to identify the very local effects deposition of dung can have on the process.**

Finally, even if the data is there, the message and the conclusions are still blurred by too many inaccuracies. Underneath, I tried to identify them and give suggestions to solve these. For instance, one issue comes from the confusion throughout the ms about “litter quality” which refers at least to two different variables (CWM litter C:N and litter quality calculated using the Decomposer Ability Regression Test proposed by Keiser et al. (2014)).

➔ **We agree that the text needed clarification and improving of readability. We put effort into correcting this. We also agree that the two meaning of litter quality were not separated enough. We clarified this point all along the manuscript and systematically specified when we refer to the "calculated quality" (from Keiser et al) or when we referred to a broader definition of quality (e.g. CWM liter C:N or overall litter quality).**

TITLE

"strongly" sounds a little vague, I would recommend to be more specific or to remove it.

"Temperate forests" sounds global while the study only took place in 3 islands in BC.

➔ **We agree with these comments and adopted a more explicit title: "Deer slow down litter decomposition by reducing litter quality in a temperate forest"**

ABSTRACT

L32 What does "latter" stand s for? Changing decomposer communities?

➔ **This part of the abstract has been rephrased to avoid such imprecisions.**

L33 Specify that this is C and N loss from litter decomposition

➔ **Done**

INTRODUCTION

This section is well written, based on recent literature and is convincing.

L74-88: You introduce the concepts of ability, litter quality and HFA, I suggest you mention the Keiser et al. paper as it is pivotal in your demonstration.

➔ **we provide additional information on the method early in the text as suggested: L129-135, L271-291**

L89-94: This section deserves to be elaborated. There are many direct and indirect (including through vegetation changes) herbivory effects on soil microbial communities and functioning, and it is unclear what you mean by "development and functioning of decomposers communities".

➔ **OK see rewrite lines 80-88.**

L106: Do we have information on how Sitka black-tailed deer diet and homeostasis compare to other ungulates? This could help to extrapolate the results.

➔ **Yes, such information exists but might not fit here especially as we refocused the text from ungulates to deer. The potential role of differences in diet and ecology among herbivore ungulates is alluded to in the discussion.**

L119: I am not sure "mechanisms" were actually assessed in this study or please cite which mechanisms were actually measured.

➔ **We removed the reference to mechanism as suggested.**

L123: Although the objectives are clearly stated hypothesis are missing here unless it is ok with PCI Ecology specifications.

➔ **We added the hypotheses associated with each objectives at the end of the introduction (see L138-145).**

M & M

L153: It would be useful to see this data (at least in Supp Mat.) and how species were distributed in PF groups as in Fig.1A.

➔ **We clarified this point by providing supplementary table S2 indicating the cover of main species along with the Shannon index and richness of each plant guild per treatment.**

L157: Unclear. 33 plant species were collected on each island or total? What are the main species? Are they all covering more than 5% of each plot area as mentioned L168?

➔ **We only sampled litter from plant species covering more than 5% of each plot, as mentioned L199. In total, we sampled 18, 20 and 17 plants species on the island without herbivory, island with an intermediate level of herbivory, and the island with a strong level of herbivory respectively. Across treatment, it corresponded to a total of 33 plant species. We better specified this point in the manuscript (see lines 198 to 202). In addition, we added supplementary table S2 that contains the name and cover details on the plant species from which litter was collected in each herbivory treatment.**

Because the experiment takes place in a forest I assume woody species are included in the litter bags, and that raises the question of the proportion of hard vs. soft litter material in each plot/litter bag? In other words did you control for the twigs quantity which might influence decomposition rates a lot?

➔ **We only used the soft tissue material for each plant species that is the senescent leaf, without including any twigs or branch from the woody species in the litter bags. Therefore, our study does not take into consideration the variation in twigs quantity across islands.**

Did you control for soil (C, N, and mass) contamination of the litter bags? And how?

➔ **Due to the very important organic layer in the soil we study we don't think that mineral pollution could occur and consequently we didn't control for contamination.**

L176-177: I suggest to group this sentence with the ones L188-190.

➔ **We grouped these sentences as suggested**

L188: Only one sampling time? So we have no idea of the dynamic of the decomposition process and the decay curve cannot be describes with confidence, neither the decomposition rate constant k can be estimated adequately. To me, this is a major limitation of this work. If not, then you should argue in the

manuscript why this is fine. Usually, 3 to 6 collections by season are needed during the first year of decomposition.

➔ **We agree that ideally it would have be interesting to study the effect of deer on the kinetic of decomposition, but aiming for repeated collection over the year would have been logistically unrealistic because of the challenges posed by accessibility and safety in winter and because this would have meant multiplying the number of litterbags accordingly which would have been unrealistic with respect to the time and resource available during a summer field season. In addition we believe that our study design is well suited to address the question we posed, namely: do deer modify C and N loss in litterbags after one year of decomposition. We indicate the reference to the time of decomposition throughout the manuscript to make this clearer. See L230-232.**

L191: In this section there is no mention of litter mass loss measurement.

➔ **We rewrote and clarified the section dealing with the calculation of litter mass loss (see L236-237)**

L198: You need to explain why you measure C and N on small mesh bags only.

➔ **We measured C and N in the small mesh bags only because of budget limitation (see L244 - 245)**

L206-214: If I am correct it means n=1 composite sample for pH, OM depth and C/N for each plot. I assume this the reason why no linear regression was tested with this data set.

➔ **Yes, you are right. Except for the OM depth, which was measured five time through each plot**

L217: You mean "(CWM) initial litter C:N ratio"

➔ **Yes, corrected in the text**

RESULTS

L251: At it stands, it is impossible to get a grasp of the diversity change (shrub, bryophytes or else) between "deer herbivory" plots with Fig.1A alone. Could you add a table in the Sup.Mat. ?

➔ **As suggest we provide a table with this (see Supplementary Table S2)**

L254: You mean the initial CWM litter CN ratio right?

➔ **Yes, corrected in the text**

Fig.1A vs. Fig 1B: so although plant species composition differed with deer browsing level, the CWM litter quality (C:N) was no different.

➔ **Yes the plant community was different but it didn't impact significantly the CWM litter C:N.**

L256-258: Nope, Soil pH does not significantly decrease according to Fig. 1D.

➔ **Actually we found a significant difference among pH means with the ANOVA but we don't have enough statistical power to discriminate which mean is different with the post-hoc test. We clarified this in the text L310-311).**

L274-275: How did you calculate the 12% and 30%?

➔ **We added the detail of this calculation in the material and method section (see lines 260 to 266).**

L281: Are these 5% significant? And how did you calculate that figure?

➔ **The 5 % were significant, and correspond to the difference in carbon loss between litter placed in plots without deer, and litter placed in plots on island with deer (Fig2C, p-value = 0.008).**

Fig.2 and Fig. S2: It would really help the reader if all panels (Ability, Litter Quality) had a different letter than HFA. I suggest A, B, C, D and E, F, G, H.

➔ **We agree and as suggested we added the letters for each panel.**

L291 Not a word on the Litter Quality diagram for N Loss?

➔ **We added this result in the manuscript (L381-384): "Similarly than for carbon, litter quality (sensu Keiser et al. 2014) was the main driver of nitrogen loss in litter bags. However, conversely to carbon, the lowest rate of N loss after one year was observed in litter from the island with intermediate browsing pressure (Figs 2E and 2H)."**

L306 Specify "Litter carbon and nitrogen loss…" "…to the initial C:N ratio…"

➔ **Corrected**

L307-308: "…litter C:N CWM…"

➔ **Corrected**

L319-322: You mean Fig. S2A and S2C (not Table S2).

➔ **Yes, it has been corrected**

L322-323: Please refer to Fig. S2B

➔ **Done**

L325: Is it really FigS2D here? I would use Fig. S2C instead. Again how did you calculate the 25% decrease? Not a word on Litter quality diagrams?

➔ **Reference to the figure has been corrected. The 25% decrease was calculated using the same formula as above ($100 - (x_{herbivory} \times 100)/x_{none}$). We added that "Litter quality had a significant effect on litter decomposition in coarse- mesh bags" to the manuscript.**

L329-330: How 31% and 47% compare to 24% and 32% respectively on L447? I am lost.

➔ **24% and 32% were a mistake and were replaced with the correct values (31% and 47%).**

L332-335: You mean significantly. I suggest you replace p= 7.89e-08 by p<0.001 here and in the whole paper.

➔ **As suggested we used p<0.001 in the whole paper.**

L337: Fig S3 B

➔ **Corrected**

L338: Fig. S3D. Is it significant/true for all islands? Based on the error bars, it seems to me that there is not much improvement at least for Strong and Intermediate.

➔ **We found an overall significant effect of feces addition but as suggested this effect is mainly due to the increased rate on the island without deer. We modified the text to explain this (see L414-426)**

DISCUSSION

L350. What about Bryant et al. 1989, Du Toit 2003, Fornara & du Toit 2007, 2008…?

➔ **We agree that our sentence was not specifying in forest ecosystems and modified the text consequently (see L441).**

L353. Could you add references of some review papers for instance to support this statement?

➔ **Done (Andriuzzi & Wall 2017)**

L356: But on Fig. 1B there are no CWN initial litter C:N differences between islands which suggest no initial litter quality differences due to herbivores.

➔ **In this sentence, we refer to litter quality as the one calculated using the method from Keiser et al (2014). It therefore refers to Figure 2D and 2E rather than Figure 1B. To better clarify this point, and as suggested by the other reviewer, we added the reference to the corresponding figures. We also modified the text to specify our utilization of the term 'litter quality'.**

L359: This sentence is really confusing. Where are these % coming from? Is it from Fig.2B and D (because you talk about litter quality, and C and N loss)?

➔ **Litter originating from the island with strong herbivory had a lower C and N loss than litter originating from the island with no herbivory. The percent are related to this observation, and are calculated following the same formula mentioned earlier ($100 - (x_{herbivory} \times 100)/x_{none}$). We re-wrote this sentence.**

L361: Based on Fig. 3, there is no much initial litter quality (C:N based) control on litter C loss (<10%).

➔ **In this sentence, we again refer to litter quality as the one calculated using the method from Keiser et al (2014). It therefore refers to Figure 2D rather than Figure 3.**

L365 or from L373 to 383: Again, I am not convinced there is such a "dramatic" change in litter quality (CWM C:N based) when looking at Fig.1. Yet, from L379 to L383, litter quality is the one on

➔ **See explanation in the next comment.**

Fig. 2B and D and indeed shows significant differences between browsing treatments. My interpretation is that browsing indeed changes plant community composition but not CWM litter C:N (Fig. 1). Therefore, it suggests that the browsing effect that you observe on litter decomposition might be related to other litter traits such as lignin or polyphenol contents, or any other litter quality parameters that you suggest in the discussion but are missing in your study. You should make this crystal clear for the reader to limit any confusion, maybe by referring to the figure in the discussion.

➔ **This interpretation corresponds exactly to what we tried to formulate in our manuscript. We acknowledge that our utilisation of the term 'litter quality', which can refer to both the CWM litter C:N and the ability of litter to decompose across soil types (calculated using the algorithm developed by Keiser et al) was confusing. In this new version of the manuscript, we attempt to clarify our utilisation of the term 'litter quality' and interpret the results in the way suggested by the reviewer's comment.**

L389 decomposability and decomposition are different things. Here you measured decomposition.

➔ **Corrected**

L392-395: As you say, the intermediate treatment CWM initial litter C:N was not different than the others on Fig. 1B. I like the suggestion that N mineralization was limited in intermediate treatment by the highest initial CWM litter C:N. I wonder also how much the increased patchiness of the browsing/pooing effects mentioned L454 in the intermediate treatment (compared to no or strong browsing) could participate to this apparent heterogeneity in C:N and N mineralization.

➔ **We agree that this is a really interesting point. However we are not sure we could totally answer this question. Concerning patchiness of browsing, it seem that the variability in litter quality (as measured by Keiser et al. 2014) coming from the intermediate browsing level presents the highest variability (Fig. 2 A and E). This is coherent with Fig. 3 where we found a large variability in original litter C:N ratio in the intermediate treatment. These two elements support our idea. Concerning patchiness in pooing we don't really find a stronger variability of decomposer ability on in the intermediate treatment (in comparison to none and severe browsing) as we would expect in a case of strong heterogeneity.**

L400: Specify you mean "litter quality" from Keiser et al. 2014.

➔ **Specified in text**

L403: Based on Fig 2B I agree with this statement. Yet, I wonder if the 5% difference mentioned L281 are related to it or not?

➔ **Yes, the 5% difference is indeed related with this statement. We clarified in the text that we calculated an average (average across litter origin), litter C loss was reduced by 5% on plots from island with strong herbivory compared to plots from island without herbivores.**

L408: pH is not different among the treatments (see Fig. 1D).

➔ **Yes, this difference between the test and figure is due to the fact that we found a significant difference among pH means with the ANOVA but we don't have enough statistical power to discriminate which mean is different with the post-hoc test. We clarified this in the text.**

L408-412: You could use some references here to support your explanation on F:B/vegetation change/deer browsing relationships. So, Ca and Mg litter contents would be nice to measure as possible explanatory litter traits of deer browsing effect on decomposition.

➔ **As proposed by the other reviewer we added a reference for this statement.**

L421: is this statement supported by Fig. 4D? If so, it is necessary to add statistics on the intertreatment effect (e.g. between sites difference).

➔ **We added the results of the post-hoc tests between treatments on Figure 4C&D, and Figure S3C&D**

I am not totally convinced by the experiment #2 . You used P. sitchensis as a standard litter but you showed that coniferous were selected/favored by deer browsing, so one could expect that Intermediate and Strong decomposer communities are also selected to decompose this litter.

➔ **Although we understand this comment we don't totally agree. We do not show that coniferous were favored by deer. Actually Picea is the least favored conifer and is among the least favored of all plant species present. But *Picea sitchensis* is a prominent tree on the studied islands and is present at all study sites. Even if the proportion of spruce in litter is higher on islands with deer in relative terms spruce litter is present everywhere and is not foreign for decomposer communities from islands without deer.**

There is also the question on how to relate data/results from pure litter (Exp.#2) vs. litter mixture (Exp#1)?

➔ **This is indeed an interesting point but it was not the objective of our study and we unfortunately can't really assess it with our design. Based on the literature we would expect an increase of decomposition rate with litter mixture. The only thing we could do is to compare average C and N loss rate between mixture and Picea alone. We found a more rapid decomposition in mixture, but this is not a proper test of the mixture effect.**

L423-424: …inclusion of the …fauna during litter decomposition affected litter mass loss at the community …". You did not calculate/show decomposition rates (k values), and Fig. S2 shows only % mass loss after one year of decomposition.

➔ **Corrected following this suggestion.**

L426 Could you elaborate on these negative effect(s)? What mechanisms? Direct or indirect?

➔ **We added in the manuscript: "This negative effect could be due to a reduction in both the abundance and the activity of the soil fauna through several mechanisms. Directly, through soil trampling by deer which might reduce soil fauna habitat through soil physical compaction and its reduction of soil pore size (Beylich, Oberholzer, Schrader, Höper, & Wilke, 2010). In addition, the reduction of litter quality by deer might be responsible for an indirect slowing down of soil faunal abundance and activity for which litter quality is known to be a controlling factor (García-Palacios, Maestre, Kattge, & Wall, 2013; Hendriksen, 1990)". L529 - 535.**

L435: Fig.S3B not S2

➔ **Corrected**

L445-446: I would use "larger proportion of N" rather than "more nitrogen". Even though dung and urine are probably more N concentrated than litter, litter quantities are way higher than dung and urine deposits, which implies that there is probably more N released by litter at the ecosystem/Island scale.

➔ **We corrected the sentence according to this suggestion**

L447: These figures do not match with the ones L329-330.

➔ **We corrected this mistake.**

L449-50: Ability? Is it related to Fig.4C and D, or Fig. 2B and C?

➔ **It is related to the Figure 2. We added the reference to the figure in the text for clarification.**

L452 "Changes" in what direction?

➔ **Feces addition was shown to increase plant productivity (Barthelemy et al 2015) and increase soil nutrient availability (Wang et al 2018). L559-561**

L453-457 Although I agree that the patchiness of the dung/urine distribution might explain the absence of high quality litter deposit effects on global decomposer ability, I wonder how much this statement questions the sampling intensity (# of litter bags deployed) and its capacity to capture the microclimatic, topographic and biochemical heterogeneity of these browsed islands.

➔ **We believe that our sampling (15 plots of 10*10m per herbivory modality) is sufficient to assess the overall effect of deer on soil despite heterogeneity. Plots were randomly located in an area corresponding to the area of the island without deer (5ha) with the rule to be far enough from one another, to be representative of the studied area.**